OBSERVATIONS *on the* PROOFS *of the* HUTTONIAN THEORY *of the* EARTH, *adduced by* SIR JAMES HALL, BART. *By* RICHARD KIRWAN, *Efq; L. L. D. F. R. S. and P. R. I. A.*

AS fome pofitions, which I laid down in my examination of Dr. Hutton's theory of the earth, may feem queftionable from the ingenious reafoning employed by Sir James Hall in the third volume of the Edinburgh tranfactions to corroborate fome of Dr. Hutton's affertions, and may even be thought inconfiftent with fome of the curious refults that occurred in the highly interefting experiments inftituted by the worthy Baronet, inferted in the fifth volume of the Edinburgh tranfactions, (a printed tranfcript of which he has had the goodnefs to fend me) I think it a duty incumbent upon me to examine both the general reafoning employed by him, and the confequences fairly deducible from his experiments; fanciful and groundlefs as the Huttonian theory feems to me to be, it may, like the refearches for the philofopher's ftone, be highly ufeful by fuggefting new experiments. Read Feb. 8, 1800.

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In the third volume of the Edinburgh tranfactions hift. p. 9, we are informed that Sir James Hall, though convinced from various obfervations that granite had once flowed in a ftate of fufion, yet acknowledged that fome difficulties accompanied this opinion; among which the moft confiderable appeared to him to be this, that in fome cafes the felfpar is feen in this ftone with its cryftals regularly defined, whereas the quartz forms a confufed and irregular mafs, being moulded on the cryftals of felfpar, whereas if the granite were formed by fufion the very contrary, he fays, fhould, it would feem, be expected; felfpar being very fufible, and quartz on the contrary highly infufible. In anfwer to which he fays "that when quartz and felfpar are mixed and pounded "together, it is well known they may be melted without diffi-"culty into a kind of glafs, the felfpar ferving as a flux to the "quartz; or the felfpar may be confidered as a menftruum in "which the quartz is diffolved; and in this view we may expect "by analogy phænomena fimilar to thofe of the folution of falt "in water. Now it is certain that when exceffive cold is applied "to falt water, the water is frozen to the exclufion of the falt; "why fhould not the fame thing happen in the folution of quartz "in the liquid felfpar, when the mafs is allowed to cool beneath "the point of congelation of the menftruum? the felfpar may "cryftalize feparately from the quartz, as we have feen pure ice "formed feparately from the falt."

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In this anſwer ſeveral particulars deſerve conſideration. In the firſt place water (to which felſpar is here aſſimilated) is never regularly cryſtallized when frozen by exceſſive refrigeration, though indeed vapour may; conſequently ſince in the preſent caſe the felſpar is ſaid to be regularly cryſtallized, the parity does not hold.

Again, to juſtify the compariſon of felſpar acting on quartz as a menſtruum, as water does upon ſalt, the felſpar ſhould always be in the larger, and quartz in the ſmaller proportion to each other, as water always is to ſalt, and this is indeed the commoneſt caſe even where the felſpar is not regularly cryſtallized; yet in Switzerland this does not happen, as Mr. Hoepfner atteſts, 4 Helvetic Magaz. p. 266; of which ſpecimens may be met in 2 Loſke Catal, Engliſh edition, p. 375, 376, No. 37, 38, 40, 41, nor in Sileſia, as Gerhard remarks, 1 Grundriſs Min. Syſtem, p. 404 and 405. How then could the felſpar have ſerved as a menſtruum or flux to the quartz in theſe caſes?

It is allowed by all obſervers that the caſes in which felſpar in granite is regularly cryſtallized are exceeding few, ſee Lentz, Emerling, Widenman, &c. Granites in which ſuch cryſtals are obſerved are called *porphyraceous granites*, and from that very circumſtance judged by many obſervers not to be ancient granites, but of modern formation, ſee 2 Widenman, p. 1005 in the note. An obſervation ſimilar to that of Sir James Hall has alſo been made

made by Mr. Beſſon in Limoges, 29 Roz. Tour, p. 89, for he diſcovered veins of granite in an argillite, though this ſchiſt did not border upon any granitic maſs, and hence he judged it of modern formation. Citizen Dolomieu alſo tells us that ſuch inſtances had occurred to him in his travels, but he thinks them perfectly diſtinct from the granite which forms granitic mountains, 16 Journ. des Mines, p. 22. Neither was Sauſſure a ſtranger to ſuch granitic veins, but he accounts for their origin very differently from Sir James, § 600.

VARIOUS attempts have been made to fuſe granites, in moſt of which, as has already been ſaid, felſpar is the moſt abundant ingredient, but in almoſt all, though finely pulverized, the quartz remained unfuſed and might be diſtinguiſhed by a lens, ſee 1 Sauſſure, §. 172, 173 and 174, 1 Gerh. Geſch. §. 51, and in the firſt part of his new mineral ſyſtem publiſhed in 1797, p. 412, and Hacquet in 1 Crell Beytrage, p. 34, 35, &c. It is plain then, that in all heats with which we are acquainted the felſpar cannot but in very rare caſes ſerve as a flux or a menſtruum to the quartz with which it is found in granites, the full proportion of quartz which can be rendered fuſible by its other component earths being already contained in the felſpar, and in fact there is no analogy betwixt water acting as a menſtruum on ſalt, and felſpar acting on quartz, for water and ſalt are ſubſtances perfectly heterogeneous to each other, whereas felſpar and quartz are both earthy ſubſtances of

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which the former contains a large proportion of the latter, as essential to its composition, and is fusible only by reason of its compound nature, but if the quantity of the quartzy ingredient be increased the whole becomes infusible, as I have experienced; whereas if the proportion of salt in water be increased, still the water will be congealable if considerably cooled; moreover quartz frequently bears the impression of stones more fusible than itself, which could not happen in any possible supposition, if all had been in a state of fusion.

AGAIN, Sir James observed, that a quantity of green glass, which had been allowed to cool slowly, was found to have lost all its vitreous properties, being opake, white and refractory; but being again melted by a blow pipe and suddenly cooled, it resumed its former properties and became glass; hence he infers that if the glass produced by the fusion of granite had been allowed to cool with sufficient slowness, it might have crystallized, producing a granite similar to the original, p. 11.

THE observation on glass here mentioned is perfectly just and has been often repeated; but the analogy betwixt this case and the formation of granite from a complete fusion of its ingredients is far from being accurate. Glass consists of a simple earth, namely, the siliceous united to an alkali. To form this union it is necessary that the integrant affinity of the siliceous particles to each other should yield to the chymical affinity

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which the alkali bears to them, and this can happen only in ſo high a degree of heat as conſiderably leſſens the affinity of the ſiliceous particles to each other. If, when this union is effected, the compound is conſiderably and *rapidly* cooled, yet the union will ſtill continue, becauſe the alkaline menſtruum being congealed, the ſiliceous particles cannot move through it to reunite to each other, though their affinity to each other in a *low* temperature be greater than their affinity to an alkali, and thus they continue in that ſtate which we call *glaſs*. Two experiments ſet this explication beyond all doubt, the firſt is, that if a ſolution of ſalt in water be ſuddenly cooled from one hundred and forty degrees above to ſix degrees below o of Fahrenheit, the whole will be congealed, and no ſeparation of the ſalt will take place, ſee 8 Nov. Comment. Petropol. p. 346. This caſe is perfectly analagous to that of glaſs. The ſecond experiment is that of Tromſdorf, 22 An. Chym. p. 115, where we find the ſiliceous particles to have ſeparated by long ſtanding (eight years) from the alkaline in a ſolution of ſilicited alkali, and to have formed perfect cryſtals hard enough to ſtrike fire with ſteel.

THAT the glaſs thus formed, being ſuffered to cool ſlowly, ſhould be decompoſed is very natural; it is what happens when certain ſalts, for inſtance nitre, are diſſolved in water to ſaturation, in a boiling heat; if the water be ſlowly cooled moſt of the nitre will cryſtallize and ſeparate itſelf. That the ſiliceous

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earth thus ſeparated ſhould be more refractory than before, ſhould be alſo expected; both becauſe it is not repulverized (at leaſt not ſtated to have been ſo) and becauſe much of the alkali, which is its menſtruum, evaporates and is volatilized during the ſlow refrigeration. But if the heat applied be much greater than at firſt, it may be vitrified a ſecond time, as more of moſt ſalts may be diſſolved in a ſmall quantity of water at 212° than at 150°.

But to reproduce granite from a general fuſion of all its ingredients by a refrigeration ever ſo ſlow, is a very different caſe from that we have juſt conſidered.

Granite is an aggregate ſtone conſiſting of quartz, felſpar and mica; of theſe the moſt fuſible is undoubtedly the felſpar, and the quartz the leaſt; let us then to indulge the worthy Baronet ſuppoſe all three in perfect fuſion in a high degree of heat, and afterwards ſlowly cooled, and thus each (though vouched by no experiment) gradually reproduced; the quartz, with the exception of the proportion thereof which enters into the compoſition of felſpar and of the mica would undoubtedly cryſtallize firſt on the ſmalleſt diminution of heat, and, being congealed in a medium ſtill in a liquid ſtate, I do not ſee why it ſhould not form regular cryſtals, which nevertheleſs ſcarce ever occur in granite except in cavities. Over this, and after a conſiderable interval of time, the mica ſhould alſo be regularly cryſtallized,

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and laſt of all, the felſpar ſhould coaleſce and congeal, (at leaſt in the Baronet's ſuppoſition) in regular cryſtals; now as the cryſtallizations of theſe three ſpecies of ſtone take place each at a diſtinct portion of time, each ſhould occupy alſo a diſtinct portion of ſpace, the firſt ſet of cryſtals being loweſt, the next over that and the laſt uppermoſt, as we find to happen when ſalts of very different ſolubility and yet in equal quantity are diſſolved and cryſtallized in water, or when ſubſtances of different degrees of volatility are ſublimed by fire. Now, among the immenſe maſſes of granite that have been obſerved and examined in various parts of the globe, not above half a dozen have occurred in which the three conſtituent parts of granite were regularly cryſtallized, very few in which diſtinct layers of each were ſeen, and *none at all* conſiſting of diſtinct regular cryſtals of each, ſuperimpoſed on each other. On the contrary in far the greater number of granitic maſſes the three above named conſtituent maſſes lie intermixed with each other in the moſt confuſed and irregular manner, and without any appearance of regular cryſtallization; inſomuch that none can ſay, from bare inſpection only, which was cryſtalized firſt, and which laſt.—nay granitic maſſes not unfrequently occur, in which it is evident that the mica muſt have cryſtallized contemporaneouſly with the quartz, for in breaking the quartzy part flakes of mica are found within it. See 6, Sauſs. § 1621.

Lastly, I muſt add, that even on the ſuppoſition that diſtinct cryſtals of quartz, felſpar and mica could be produced by fuſion,

fusion, they still would be far from resembling those we are acquainted with, which essentially contain some particles of water, as I have elsewhere shewn.

PERHAPS some may say that the same difficulties occur in accounting for the crystallization of granite in the moist way; on mature consideration however it will readily be seen, that the causes of coadunition in the dry and the moist way are very different, and that their effects should also be different. For supposing the earths, that enter into the composition of granites, dissolved in the moist way, their precipitation and imperfect crystallization may be ascribed to the union they contract with each other forming masses of each of the constituent ingredients of granite, which water can no longer hold suspended; hence the precipitation of each of the three species of stone is nearly *contemporaneous*, whereas if the formation of these ingredients should take place in the dry way, it would necessarily be *successive*, keeping pace with the successive diminutions of heat, and then the abovementioned consequences would naturally ensue.

THE state of the granitic ingredients in fusion which I have above given agrees pretty nearly with that presented by Sir James himself; he supposes the quartz, felspar, shorl, mica, garnet, &c. melted together, and the most fusible of them to be the menstruum in which the rest are dissolved, and that they differ from each other in their properties of solution as salts

differ from each other. Some of them being more ſoluble in the menſtruum when very much heated than when it is comparatively cold, and others may be ſoluble in it when little warmer than its point of congelation.——" If then we ſay, for inſtance, " that the congealing point of the ſolvent is 1000° of Fahrenheit " and if the ſolution is at the temperature of 2000°, we may " conceive one portion of the matters diſſolved as held by the " ſimple diſſolving power of the menſtruum, and another as " held by means of its elevated temperature; when therefore " a maſs of this kind is allowed to cool very ſlowly thoſe ſub- " ſtances held in ſolution by the heat of the ſolvent will firſt " ſeparate, and being formed in a liquid will aſſume their cryſ- " taline form with perfect regularity."—This conſequence is truly deduced from the Baronet's hypotheſis, but being contrary to fact diſcovers the falſhood of that hypotheſis, for if any of the fore-mentioned component parts of granite can be ſaid to be held in ſolution by the high heat of the ſolvent, it is ſurely the quartz; now the quartz is ſcarce ever found regularly cryſtallized when forming a component part of granite, as all mineralogiſts atteſt and is matter of univerſal obſervation.

But the Baronet continues, " whereas thoſe ſubſtances which " were held by the menſtruum ſimply as a fluid, will not ſepa- " rate until the congelation of the ſolvent itſelf takes place, " when the cryſtals of the various ſubſtances will intermix and

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" confound the regularity of form which each would have af-
" fumed if left to itfelf. In this manner one of the common
" kinds of granite will be produced confifting of perfect cryftals
" of fhorl, mica or garnet inclofed in a confufed mafs of felfpar,
" quartz and fhorl."—This conclufion is as objectionable as the foregoing; for not to mention that granites, in which fhorl and efpecially garnets are found, are far from being common, affuredly fhorl and garnet approach more to the fufibility of felfpar (the fuppofed menftruum) then either quartz or mica. Thefe therefore are thofe which fhould cryftallize without any regular form in the Baronet's hypothefis, and not the quartz and mica; which is juft the contrary of what he himfelf has obferved, for he tells us, p. 9, " he found the cryftals of felfpar regularly
" defined."

Sir James has fince very wifely declined juftifying his theory of the formation of granite by fufion, and by the advice of Doctor Hope very properly applied himfelf to experiments on various fpecies of *whin*, a denomination which in Scotland comprehends grunftein, bafalt, trap, wacken and porphyry, ftones in which, except the laft, none of the component ingredients are found regularly cryftallized, and on the laft he has made no experiment.—The former he tells us were foftened or fufed in a heat of from 38° to 55° of Wedgwood, the glaffes to which they were reduced were foftened on a range of from 15° to 24, and the

the maſſes of the original ſtony appearance, to which thoſe glaſſes were reduced by ſlow cooling, were ſoftened in degrees of heat of from 32° to 45°; to the formation of theſe laſt he conſtantly applies the term *cryſtallization* and calls them *cryſtalites*. To the vague term of cryſtallization I muſt however object, for as thoſe ſtones in their original ſtate preſent no regular cryſtals, but are at moſt internally and imperfectly cryſtallized, ſo they muſt be when reduced from a glaſſy ſtate to one reſembling their original and thus diſcover rather a *niſus* towards cryſtalization than perfect cryſtals, which latter the term cryſtallization generally applied would lead us to expect.

BEFORE I proceed to the detail of theſe experiments, I muſt obſerve that the different fuſibilities of theſe *cryſtalites*, as he calls them, indicate a very different ſtate from that in which they originally exiſted; the former requiring a heat of from 32° 45° and the latter a heat of from 38° to 55°, the reaſon of which is eaſily diſcovered when the two ſtates are deduced from a different origination, but is in vain ſought for, when both are to be deduced from one and the ſame origin.

PASSING over the general preliminary accounts of theſe experiments, which are to be found from p. 7 to p. 10 of this diſſertation, I ſhall now examine the moſt important particulars of each, as far as they give occaſion to any ſtriking obſervations. In this examination I am much aſſiſted by the ingenious, accu-

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rate and ſkilfully conducted analyſis of Doctor Kennedy, who bids fair to rival the excellence attained by the greateſt maſters of that ſublime and difficult art.

Experiment the firſt.

Grunſten a compound of felſpar and hornblende intimately mixed with each other, was the ſubject of this experiment. Its *colour*, black or greeniſh black intermixed with pale reddiſh brown; both the felſpar and hornblende imperfectly and confuſedly cryſtallized in minute grains; the *fracture* partly ſtriated and partly foliated. *Luſtre* moderate, its hardneſs 7, or almoſt 8, gives an earthy ſmell when breathed upon, and frequently contains ſmall ſpecks of pyrites, ibid. p. 7.

THIS ſubſtance he vitrified by a ſtrong heat and ſubſequent rapid cooling, p. 9. A fragment of the glaſs thus produced being introduced under a narrow muffle and heated to 21° in one minute became ſo ſoft as to yield readily to the preſſure of an iron rod, but after a ſecond minute it became quite hard though the temperature had been ſtationary; the ſubſtance thus hardened underwent a thorough change, it loſt its vitreous character, its fracture was like that of *porcelain* (that is even) and it was fuſible only in a heat of 31° p. 11. In another experiment, (ibid.) he found this change to take place even before the glaſs was in perfect fuſion; for while both ends of a fragment of this glaſs were

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ſupported on reſts of clay, it was found not to ſink down between them until the heat was raiſed to 30°—In another experiment he found the conſolidation which he (improperly as I think) calls cryſtallization, to take place even while the heat was gradually increaſed, and the ſubſtance ſtill ſo viſcid as to retain the original ſhape of the fragments.—In another experiment where the glaſs was ſlowly cooled its texture was found completely to reſemble that of whinſtone, the *fracture* was *rough*, *ſtony* and *cryſtalline*, with a number of ſhining facettes interſperſed through the maſs and a few cryſtals in the cavities produced by air bubbles, p. 8.

These experiments may be conſidered in two points of view, firſt with reſpect to phænomena of conſolidation in a heat either gradually increaſed *above* or gradually diminiſhed *below* the heat neceſſary to ſoften the vitreous ſubſtance, the loſs of the vitreous character and the *ſtony* appearance aſſumed through ſlow refrigeration.

And in the ſecond place we may examine how far the phænomena here obſerved tend to countenance the Huttonian theory either of the formation of granite, trap or baſalt or other ſtony ſubſtances.

In this reſpect only it concerns me to examine theſe experiments, yet I cannot forbear mentioning ſome few reflections on the firſt.

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It has been obſerved by all thoſe who have attended to the formation of common glaſs (and is indeed evident from the fumes that float over its ſurface) that from the inſtant it enters into fuſion, it is in a conſtant ſtate of decompoſition, gradually becomes leſs fuſible and increaſes in denſity; the ſubſtances that thus eſcape are, in this caſe the ſaline, as Boſc D'Antic has ſhewn and Maquer alſo aſſerts, (See 1 Boſc D'Antic, 10 and 242, 213.) and hence the loſs of weight which glaſs thus ſuffers, ibid. 220. and 4 Maquer, 261; Maquer alſo obſerved that glaſs kept too long in fuſion looſes its tranſparency and becomes opake, becauſe the flux evaporates; and he obſerves that glaſſes formed of argil, lime and gypſum, are particularly ſubject to this accident. Lavoiſier noticed the ſame phænomenon during the fuſion of felſpar even by oxygen air, namely, that the longer it was kept in fuſion the more infuſible it became, Mem Par. 1783, p. 577, which he imputed to the volatility of one of ſome or other of its ingredients; and he afterwards found occaſion to extend the ſame remark to ſteatites and alſo to a mixture of equal parts of quartz and calcareous ſpar; this increaſed infuſibility of certain ſubſtances by a gradually increaſed or continued heat is not therefore a new diſcovery, having been already noticed; but Sir James Hall has conſiderably inlarged it, by ſhewing that the ſtones he operated upon had reaſſumed their ſtony appearance, after having been in a vitreous ſtate; this appearance, if I under-

ſtand him rightly, they have aſſumed only in conſequence of ſlow cooling, and not merely by a heat either ſtationary or gradually increaſed; conſolidation only being the effect of ſuch treatment.

THIS conſolidation, Sir James calls *cryſtallization*, a term which ſeems to me highly improper; for according to every ſenſe in which this term has ever been employed, whether that operation was perfect or confuſed, it denotes at leaſt an union of particles previouſly diſperſed through a liquid medium; they muſt therefore be at liberty to move through this medium in order to coaleſce and re-unite to each other; if both they and the medium itſelf coaleſce and conſolidate, this action is called *coagulation*, as happens in what was called the *offa helmontii* and the jelly formed by the *liquor ſilicum*; but in Sir James's experiment we find the conſolidation to take place in a fragment of glaſs, which ſtill retained its ſolid ſtate, and conſequently the particles were not at liberty to move towards each other; this conſolidation muſt therefore evidently have ariſen from ſome internal change in the conſtitution of the glaſſes in which it was obſerved; what theſe changes may have been I ſhall now examine: In the firſt place it is highly probable that ſilex, argil and lime, and ſlightly oxygenated calx of iron, whatever be their affinity to each other when duly proportioned, require like all ſolids to abſorb in their paſſage to a liquid ſtate a certain portion of latent heat; but when

when in fusion and the particles of each chymically united, they require a higher degree of heat to keep them in fusion, their elective affinities promoting fusion before the union, and impeding it after the union is formed; it is thus that iron and platina, metals separately highly infusible, contribute to each others fusion; but when fused, become still more infusible, as appears by Rinm. § 135; sulphur and lead are separately and easily fusible, but when united their fusion becomes much more difficult.

Again, Dr. Kennedy has discovered that all these whins contain ten per cent. of soda, and Vauquelin has lately discovered tartarin in felspar; in the high heats to which these stones are exposed in order to vitrify them, may it not be supposed that these salts are in some measure volatilized and the compound thus rendered less fusible? Though in an high heat rapidly produced, they may still be fusible, as a smaller proportion of soda will in that circumstance suffice to that effect.

The next circumstance to be accounted for is the saxification or *stony* appearance assumed by the vitrified stones when slowly cooled, by far the most curious fact, for which we are indebted to the ingenuity of Sir James. To account for this change it is proper to remark that though whins are said to be vitrified in a high degree of heat, yet this is not rigorously true, for in that

cafe they fhould afford a tranfparent glafs whofe fracture would be perfectly polifhed with a ftrong luftre, as we fee that of common glafs, whereas in truth they melt only into an *enamel* nearly approaching to the perfect vitreous ftate; even the bottles made of them are nothing more; and hence their fuperior hardnefs. Their ingredients therefore are not uniformly diffufed through their whole mafs, but lie in the fame order and pofition as before fufion, and in effect they contain much more filex than can be completely vitrified by the fmall proportion of lime and argil that enter into their compofition, even though affifted by the foda; and in the next place we muft notice that the affinity of foda to filex diminifhes in the fame ratio as the heat diminifhes and confequently they feparate, if the heat be not fo fuddenly diminifhed as to impede all motion. This is evident by what happens to common glafs when flowly cooled down to the temperature of the atmofphere, as came to pafs in the glafs houfe at Leith, and conformably to this inftance the faxification might take place even after a perfect vitrifaction. That there are unions grounded on chymical affinity which take place to a certain degree only at certain temperatures and are in great meafure loofened at a lower temperature, appears in the common inftance of the folution of moft falts in water, fpirit of wine or other menftruum greater in a high degree of heat than at a lower, and has alfo been noticed

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in the fuſion of gold in a maſs of ſilver, for if the ſilver be very gradually cooled the gold will ſeparate from it, as Homberg obſerved, Mem. Par. 1713.

Now the affinity of ſilex to the alkali, being looſened by a ſlight diminution of heat, the affinity of argil to the ſilex to which it united only as to a *compound* in the given temperature, is alſo neceſſarily looſened. That in the dry way argil unites to ſilex in temperatures below 150°, only in conſequence of the previous union of the ſilex to the alkali, is clearly deduced from this fact, that if the alkali be abſent the union will not take place in temperatures below 150°, whereas it takes place by Sir James's own experiment at temperatures below 100° when the alkali is preſent, for he found the whins fuſible at 55°. It is true the whins contain lime alſo, but though the preſence of a certain proportion of lime contributes materially to the fuſibility of ſilex and argil, yet it would be ineffectual in degrees of heat below 120° if an alkali were not preſent to aſſiſt it, as I know by experience.

The preſence of argil contributes alſo to the diminution of the affinity of the alkali to the ſiliceous ingredient, as the alkali ſeems to have nearly as ſtrong an affinity (ſome think ſtronger) to argil as to ſilex; hence it is that all Analiſts ſince Bergman's time employ an alkali to looſen the intimate union of ſilex and argil in precious ſtones.

These

Thefe facts being duly confidered, we fhall not be furprifed at feeing the clofe vitreous texture deftroyed by the flow cooling of melted whins (all of which contain the above ingredients) and fucceeded by the loofer texture of a mere ftony fubftance. This is the only change that takes place, if we except the minute and indeterminate cryftallizations that occur in the cavities formed by the expulfion of air while the mafs was as yet foft, for the facettes interfperfed through the ftone cannot be accounted cryftals, but only the rudiments of cryftallization. Thefe are formed at the inftant the affinity of the alkali is leffened and the earths begin to affume their folid ftate. The alkali being as yet liquid, allows the earthy particles to move through it, and to form thefe incipient cryftallizations.

We are now to examine how far the ftony ftructure affumed through flow refrigeration, by ftones previoufly fufed, tends to afford any fupport to the Huttonian theory. In my opinion it affords none at all; the utmoft effect it can produce in an unprejudiced mind is to render the origin of whins ambiguous by making them affume the appearance of a Neptunian origin, when in fact they owe it to fufion; but it is only an *appearance*, for natural whins are accompanied with circumftances and contain fubftances which contradict that appearance, and prove it to be deceitful. Befides, thefe experiments have no relation whatfoever to granite or calcareous maffes which form the bulk of the

the globe, and afford not the ſlighteſt indication of their origin; whins, though they abound in Scotland and ſome other countries, are in compariſon of the former but thinly ſcattered over the ſurface of the globe. Some reſemblance betwixt them and lavas has been long noticed. I ſhall now briefly mention a few of the diſcriminating characters of the artificial and natural whins, which may in moſt caſes prevent us from confounding them, or aſcribing to them a common origin.

1° The *natural* whins, particularly amygdaloids (vulgarly called toadſtones) frequently contain calcareous ſpar and zeolyte; now as the former contains fixed air, and the latter a notable proportion of water, I hardly think Sir James, who profeſſes not to agree with Dr. Hutton in all points, will allow theſe to have been vitrified or fuſed.

2° The *natural* whins, according to Dr. Kennedy's ſtatement, loſe five per cent. of water and other volatile matter when heated to redneſs. It is not ſaid whether the artificial loſe any part of their weight by ſuch treatment, but it is plain they would not, ſince even the lavas of Catania and Piedemonte, though of ancient date, loſt none, as Dr. Kennedy expreſsly notices, and has thus afforded an excellent criterion for diſtinguiſhing the long conteſted origination of theſe ſubſtances.

3° As

3° As Sir James has neglected giving a *complete* account of the external characters of the natural whins, which were the ſubject of his experiments, as alſo of the regenerated or artificial whins derived from them, and as I have not myſelf ſeen them, it is difficult for me to compare them with each other, and would indeed be impoſſible if ſome account of them had not been given by Mr. Pictet in his valuable Journal Britannique, copied into the 5th Vol. of the new Rozier's Journal, p. 313. It is the reſult of the examination both of the natural and artificial whins by the Society of Natural Hiſtory at Genève.

As to the natural grunſtein, No. 1. they remark that it betrays not the leaſt mark of an igneous origin, but that the whins which Sir James produced from it had every diſtinctive character of a *lava*, and even of a *porous lava*.

The baſalt (or rather trapp) on which the caſtle of Edinburgh ſtands is of a compact ſtructure; the artificial produced from it, Sir James tells us, ſo greatly reſembles it both in colour and texture that it would be difficult or perhaps impoſſible to diſtinguiſh them, *but for a few minute air bubbles, diſtinguiſhable in the artificial.* Neptuniſts will however conſider this as a leading character of diſtinction. The mineralogiſts of Geneva add, that the colour of the artificial is deeper, and its hardneſs greater than that of the natural. If the ſpecific gravity and other

characters

characters of both were given, it is probable that other differences might be perceived. It is only in these characters that any difference can be expected, as the internal composition must be the same in both.

Of the remaining artificial whins I can give no account, their external characters having been omitted; I cannot however pass over the general inferences that Sir James deduces from his experiments, namely, "that the arguments against the subterraneous "fusion of whinstone, derived from its stony character, seem "now to be fully refuted," for not to repeat what has been already said, that many of them contain substances whose existence is incompatible with that hypothesis, I must farther add that the upright state in which many of them exist, for instance, the basaltic pillars of Staffa, and of the Giants Causeway, and of many other countries, the basis they rest on, sometimes granite, sometimes gneiss, sometimes coal or limestone, and the total absence of all signs of the operation of fire, forbid us to entertain any doubt of their production in the moist way. Nay the college of Dublin now possesses fragments of basaltic pillars in which marine shells are imbedded; if such evidence can be resisted it is in vain to seek for greater.

Sir James thinks the cause of the fluidity of lavas, which I formerly suggested, as strange and inconceivable as that of Citizen

 Dolomieu

Dolomieu. Not having had the happineſs of viewing thoſe ſtupendous torrents, I founded my opinion on the accounts given by the moſt accurate obſervers, and particularly of Citizen Dolomieu who beheld and carefully examined every circumſtance relating to them for many years. This great obſerver has not thought my opinion ſo inconceivable, for he has ſince embraced it; "from the "manner, ſays he, in which lavas flow, it cannot be doubted "but they carry with them a ſubſtance capable of maintaining "their heat and fluidity, and contain a ſubſtance which burns in "contact with the atmoſphere until it is conſumed. This ſub-"ſtance, of which ſulphur is at leaſt one of the principal ingre-"dients, if it be not the only one, bears a ſtrong reſemblance in "its conſtitution to phoſphorus, being capable of two ſorts of "combuſtion. This combuſtion ſeems capable of maintaining "fluidity in a bed of lava, &c." 1 New Rozier's Journ. p. 119 and 120.

Sir James ſays I have ſuppoſed ſubſtances that have left no trace of their exiſtence. Other obſervers however diſcovered theſe traces, as Dolomieu and Fabroni in the paſſages I have already quoted. Mineralogy, vol. 1. p. 397, and 1 New Roz. p. 120, 121. It is not to be expected that volatile ſubſtances, ſuch as ſulphur and petrol, ſhould long remain.

However,

However, I acknowledge that the caufe of the ftony appearance which lavas after cooling exhibit, difcovered by Sir James, appears to me at prefent by far the moft probable; and that in this refpect his difcovery is of great importance to geology. But I perfift in thinking his experiments afford no confirmation of the high degrees of heat attributed to volcanos, and ftill lefs to the many hypothefes gratuitoufly heaped on each other by Doctor Hutton, or to the volcanic origin of whins or traps, for the reafons already affigned.